

FUNCTION TRANSFER IN HUMAN OPERANT EXPERIMENTS: THE ROLE OF STIMULUS PAIRINGS

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Although function transfer often has been studied in complex operant procedures (such as matching to sample), whether operant reinforcement actually produces function transfer in such settings has not been established. The present experiments, with high school students as subjects, suggest that stimulus pairings can promote function transfer in conditions that closely approximate those of matching to sample. In Experiment 1, the subjects showed transfer of operant responding from three geometric figures (C1, C2, C3) to three colored shapes (B1, B2, B3) when the latter were paired with the former. Experiment 2 involved two groups of subjects. In the matching group, subjects matched the colored shapes with the geometric figures; in the yoked group, the shapes were merely paired with the geometric figures, and the schedule of stimulus pairing was yoked to the performance of the subjects in the matching group. Both groups of subjects showed function transfer. Experiment 3 documented function transfer from C stimuli to B stimuli through indirect stimulus pairings (A–B, A–C). In Experiment 4, function transfer was obtained even though the subjects vocalized continuously during the pairing trials, presumably preventing covert verbalization that might mediate transfer effects. Our results are consistent with a Pavlovian account and raise difficulties for current operant theories of function transfer.

Key words: function transfer, Pavlovian conditioning, matching to sample, computer mouse click, key press, humans

The term *function transfer* describes the fact that the behavioral functions of stimuli sometimes spread to other stimuli in spite of not having been trained in their presence (see Dougher & Markham, 1994, 1996). Although the empirical and theoretical importance of function transfer is now widely accepted (e.g., Dougher & Markham, 1994, 1996; Dymond & Rehfeldt, 2000; Friman, Hayes, & Wilson, 1998; Tonneau, 2001; but cf. Sidman, 2000), its explanation has remained controversial. Part of the difficulty in formulating a coherent account of function transfer is the variety of procedures and settings in which function transfer has been studied.

In behavior analysis, function transfer most often has been studied in relation to *stimulus equivalence*, originally defined as the emergence of reflexivity, symmetry, and transitivity

in conditional discrimination tasks (see Sidman & Tailby, 1982). Because studies of stimulus equivalence have documented the transfer of numerous functions of stimuli matched directly or indirectly to one another (e.g., de Rose, McIlvane, Dube, & Stoddard, 1988; Gatch & Osborne, 1989; Hayes, Kohlenberg, & Hayes, 1991; Lazar, 1977; Lazar & Kotlarchyk, 1986), some researchers have argued that stimulus equivalence can produce, combine with, or otherwise account for function transfer in complex operant settings (e.g., Sidman, 1994, 2000). For instance, Hayes et al. (1991) wrote that “equivalence relations would be of limited ultimate importance . . . if these relations did not combine with other psychological processes” (p. 119). As an example of such combining, these authors mentioned “the transfer of functions through equivalence relations” (p. 119). Dougher, Augustson, Markham, Greenway, and Wulfert (1994) similarly referred to function transfer as “the acquisition of stimulus function by virtue of membership in an equivalence class” (p. 331), and Sidman, Wynne, Maguire, and Barnes (1989) suggested that “the most important function of equivalence relations is to transfer new stimuli—for example, words—into already existing functional classes” (p. 273).

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A problem with these arguments is that function transfer has been demonstrated in settings in which reflexive, symmetric, or transitive behavioral relations cannot be defined coherently (Tonneau, 2001). Some of the relevant studies (see Zentall, Galizio, & Critchfield, 2002) document function transfer through common stimulus choices (e.g., Urcuioli, Zentall, & DeMarse, 1995; Wasserman, DeVolder, & Coppage, 1992), common outcomes (e.g., Edwards, Jagielo, Zentall, & Hogan, 1982), or common experimental treatments (e.g., Vaughan, 1988). In each of these studies, pairing different stimuli with similar antecedent or subsequent events promoted function transfer from one stimulus to another (Dougher & Markham, 1996; Hall, 1996).

A second problem for operant, stimulus-equivalence accounts of function transfer (e.g., Sidman, 2000) is the extensive documentation, starting with Pavlov (1927), of transfer of functions through stimulus pairings and in the apparent absence of operant reinforcement. In Pavlovian conditioning, elicitation functions may be said to transfer from the unconditional to the conditional stimulus (Mackintosh, 1983, pp. 68–74; Turkkan, 1989, 1993). But the stimulus pairings of Pavlovian conditioning also have been shown to transfer Pavlovian reinforcement (e.g., Leyland, 1977; Rashotte, 1981), operant reinforcement (Williams, 1994), and occasion setting (e.g., Holland & Forbes, 1982). Other Pavlovian experiments have documented function-transfer effects similar to those observed after stimulus-equivalence training in humans (e.g., Dougher et al., 1994). In three experiments by Honey and Hall (1989), for example, pairing two stimuli A and B with the same consequent stimulus C promoted generalization of conditioned suppression from A to B, demonstrating function transfer through indirect pairings of the A–C, B–C type (see also Ward-Robinson & Hall, 1996).

One way to unify these lines of research is to assume that function transfer is driven by networks of stimulus pairings, independently of reinforcement (Boelens, 1990; Rehfeldt & Hayes, 1998; Staats, 1966; Tonneau, 1993). From this perspective, the operant experiments that are said to document function transfer “through equivalence relations” (Hayes et al., 1991, p. 119) or “across mem-

bers of an equivalence class” (Catania, Horne, & Lowe, 1989, p. 99) produce their results because of the stimulus pairings implicit in the operant, stimulus-equivalence tasks, and not because of the operant components of such tasks.

This hypothesis is supported by studies of function transfer in humans that rely on the paired-associate paradigm (see Rudy, 1974) or preparations analogous to conditioned suppression (Arcediano, Matute, & Miller, 1997). Further support comes from experiments that manipulate stimulus contiguity within operant discrimination tasks and produce function transfer (e.g., Boelens & Smeets, 1990; Smeets, 1994; Smeets, Barnes-Holmes, & Nagle, 2000). The studies of Leader, Barnes, and Smeets (1996), Leader and Barnes-Holmes (2001), Leader, Barnes-Holmes, and Smeets (2000), and Smeets, Leader, and Barnes (1997), in which human subjects spontaneously matched stimuli paired directly or indirectly with one another, provide additional evidence that human behavior is highly sensitive to stimulus pairings.

Although considerable evidence across species shows that function transfer is governed by stimulus pairings (Turkkan, 1989, 1993), most of the relevant experiments in humans confound stimulus pairings and other variables. In matching-to-sample studies that involve stimulus compounding (e.g., Smeets et al., 2000; Stromer & Stromer, 1990), for example, operant reinforcement is employed during training. In the studies of Boelens and Smeets (1990) and Smeets (1994), which document function transfer without operant reinforcement in the compounding phase, the responses measured during the transfer tests (pointing or touching) also were emitted during stimulus compounding; hence function transfer could be explained in terms of stimulus-response contiguity (e.g., Gardner & Gardner, 1988). Finally, the results of Leader and colleagues (e.g., Leader et al., 1996) can be interpreted as function transfer only by assuming that the subjects entered the experiments with a preestablished repertoire of identity matching. If human subjects tend to choose B in the presence of B, for example, and if this preestablished function transfers through stimulus pairings, then after direct or indirect AB pairings the subjects will choose B in the presence of A. Because Lead-

er and colleagues did not train identity matching or any other particular behavior; however, the role of function transfer in their experiments remains uncertain.

In the human operant literature, only one report (Markham & Markham, 2002) provides unambiguous evidence of function transfer in the absence of explicit operant reinforcement and of the target response during pairing. This report, which extended the Pavlovian study of Honey and Hall (1989) to human subjects, documented the transfer of discriminative functions from one stimulus to another through pairing with a common stimulus. Markham and Markham's use of electric shock as one of the stimuli, however, makes it difficult to compare their design with more typical operant studies of function transfer in humans (e.g., Catania et al., 1989).

The present work was designed to further investigate the role of stimulus pairings in such studies. The effects of operant conditional discriminations and stimulus pairings were compared in similar experimental procedures. Pairing proceeded in the absence of operant reinforcement, and stimulus functions were trained explicitly instead of being merely assumed. The pairing situations were otherwise designed to approximate the matching-to-sample training of most studies of function transfer in humans (e.g., de Rose et al., 1988; Hayes et al., 1991). The results prompt a reconsideration of Pavlovian and operant accounts of function transfer.

EXPERIMENT 1

The purpose of the first experiment was to document the transfer of discriminative function through stimulus pairings in conditions as simple as possible. In contrast to paired-associate research (e.g., Rudy, 1974), and consistent with most operant studies of function transfer, only a limited number of response options was used. This study also served as a convenient point of reference for the following experiments.

METHOD

Subjects

Six high school students (3 boys and 3 girls, with ages ranging from 12 to 14 years) vol-

unteered as subjects. The subjects' names were coded as P1 through P6.

Apparatus

Each subject worked individually in one of four rooms. Each room was equipped with a computer, a table, and a chair. Stimuli were presented via the computer's monitor (800-by 600-pixel resolution). Responses consisted of pressing keys on the keyboard; their latencies were registered with an accuracy of 0.01 s. Programming was done in Delphi 5.0TM.

Procedure

The experiment was completed in one session, about 30 min in duration, divided into a short phase of pretraining and a longer phase of training and testing.

Pretraining. The experimenter accompanied each subject to an experimental room and read the general instructions. The following instructions (in this article all instructions and written prompts have been translated from Spanish) were present on the screen at the start of the session:

Hello! Thanks for participating. This experiment is not a test of intelligence or personality but is a study on learning in general, and your task is quite simple. From time to time, the computer will ask you to press a key (A or G or L). If you don't know where these letters (A, G, and L) are on the keyboard, look for them right now, because you'll need to know where they are later. You'll also need to know how to use the mouse to move from one screen to the next. Now, click with the mouse on the rectangle labeled "Ready" to see the next screen.

A rectangle (7 mm by 23 mm) labeled "Ready" was visible at the bottom of the screen. When the subject clicked on the rectangle, the former instructions were replaced by the following:

Usually, after pressing a key the computer will tell you whether you were right or not. Your task is simple, but every error counts. It is better to respond SLOWLY and carefully than to respond quickly and make a mistake. So TAKE YOUR TIME BEFORE ANSWERING. What is important is to STAY FOCUSED, TO READ CAREFULLY WHAT THE COMPUTER SAYS, and to avoid mistakes. Even if you make mistakes at first, you'll learn the correct letters in the course of the experiment. Click with the

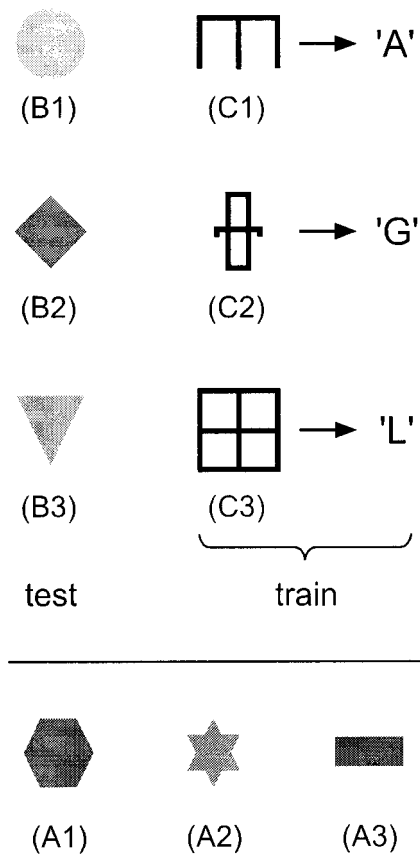


Fig. 1. Stimuli used in the experiments. C1, C2, and C3 were black; B1, B2, and B3 were yellow, pink, and blue, respectively; A1, A2, and A3 were green, gray, and red, respectively. Each stimulus was centered on an area of 80 by 80 pixels on the computer screen and measured about 2 cm by 2 cm. In all experiments, choosing the letters A, G, and L was trained in the presence of C1, C2, and C3, respectively.

mouse on the rectangle labeled "Ready" to start the experiment.

The experimenter stayed with the subject while reading these instructions and answered questions by rereading the relevant portions of the instructions. She then announced that the session would be over in about 30 min and left the subject alone in the room. When the subject clicked on the rectangle labeled "Ready" the pretraining phase began.

The aim of pretraining was to teach the subject to press a different key (A, G, or L) in the presence of the geometric figures labeled C1, C2, and C3, respectively, in Figure 1. The letters A, G, and L were used because

they are equidistant on the central row of keys on a standard keyboard; the subject was to press A in the presence of C1, G in the presence of C2, and L in the presence of C3.

Pretraining proceeded in discrete trials. Each trial started with one of the figures (C1, C2, or C3) at the center of screen, accompanied by a prompt at the top of the screen: "Choose the letter A or the letter G or the letter L." When the subject pressed one of these keys, the chosen letter appeared on the screen 2 cm above the geometric figure. Also, the prompt vanished and was replaced by a message commenting on the subject's choice (either "Good!" or "Wrong. Try again," depending on whether the chosen letter was correct or not). After 2 s, the screen went blank for a 1-s intertrial interval.

A correction procedure was used throughout pretraining so that after an incorrect response, the same trial configuration was repeated until the subject chose the correct key twice in a row. Pretraining proceeded in blocks of three trials involving C1, C2, and C3 in random order, and ended when the subject made no incorrect response on any such block.

Training and testing. The training-and-testing phase started with these instructions:

Now, apart from pressing keys, you'll need to LOOK at some figures that the computer will show you on the screen. You'll need to pay attention. Click with the mouse on the rectangle labeled "Ready" to see the remaining instructions.

The remaining instructions warned the subject about the occasional use of nonspecific feedback during training and testing:

Until now, every time you chose a letter the computer told you whether you were right or wrong. However, starting now, from time to time the computer WILL NOT TELL YOU. You'll need to guess whether you were right or not. Even though the computer will not tell you anything, it will REGISTER YOUR RESPONSE. We'll give you your results at the end of the experiment. When you want to start, click with the mouse on the rectangle labeled "Ready."

When the subject clicked on the rectangle labeled "Ready," the training-and-testing phase began. This phase was divided into 16 cycles of three training trials (pending correction:

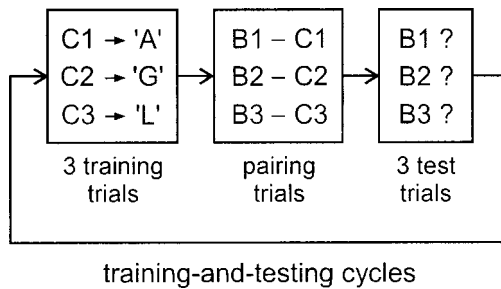


Fig. 2. Structure of the training-and-testing phase of Experiment 1. This phase included 16 cycles of training and testing, each comprised of three training trials, six pairing trials, and three test trials. Test trials evaluated a possible transfer of discriminative function from C1, C2, and C3 to B1, B2, and B3. See text for additional details. Note: The pairings shown here represent only one possible assignment of C1, C2, and C3 to B1, B2, and B3; in the experiment, the six possible assignments were counterbalanced across subjects.

see below), six pairing trials, and three test trials (see Figure 2).

The three *training trials* of each cycle were identical to those in pretraining, except that response-specific feedback was delivered only with a probability of .833 per trial. With a probability of .167, the following words appeared on the screen: "You'll know your results at the end of the experiment." Providing this nonspecific message with a low probability was done to facilitate generalization from training trials to test trials (on which the nonspecific message was always used). Training trials with the nonspecific message proceeded without correction. On the other training trials, the same correction procedure as in pretraining was used. The order of the training trials on each cycle was random.

The six *pairing trials* of each cycle associated C1, C2, and C3 with the colored shapes labeled B1, B2, and B3 in Figure 1 (a yellow circle, a pink diamond, and a blue triangle). On each pairing trial, a colored shape (B1, B2, or B3) appeared at the center of the screen with the word "look" 3 cm below it. One of the geometric figures (C1, C2, or C3) was added 1.5 s later, 1.5 cm above the colored shape. The word "look" and both stimuli remained visible for 1.5 s; then the screen went blank for a 2.5-s intertrial interval. The order of the pairing trials was random within blocks of three. There are six ways to pair three colored shapes (B1, B2, B3) to three

geometric figures (C1, C2, C3); each possible way was assigned to a different subject.

The three *test trials* that ended each cycle were designed to evaluate a possible transfer of discriminative function. On each test trial, a colored shape (B1, B2, or B3) appeared at the center of the screen while the subject was prompted to press A, G, or L. Once the subject pressed one of these keys, the chosen letter appeared on the screen 2 cm above the colored shape, and the prompt was replaced by the message: "You'll know your results at the end of the experiment." After 2 s, the screen went blank for a 1-s intertrial interval. The order of the test trials on each cycle was random.

Every fourth cycle of training and testing was followed by a self-paced break to allow the subject to rest. Each break started with the following instruction:

The experiment is not over yet. However, you may rest a little if you wish before continuing the experiment. When you want to continue, click on the rectangle labeled "Ready."

When the subject clicked on the rectangle labeled "Ready," training and testing resumed. At the end of the session, the message "The experiment is over" appeared on the screen. The subjects were debriefed collectively once the entire series of experiments was over; meanwhile they were instructed not to discuss the study with their schoolmates.

RESULTS AND DISCUSSION

The messages "Good!" and "Wrong. Try again" appeared to be effective consequences. Subjects averaged 9.33 pretraining trials (range, 3 to 15), including corrections, to reach three correct key presses in a row. Figure 3 presents the percentages of correct responses on training and test trials for each subject. Correct responses were defined on training trials as the responses trained by the experimenter, and on test trials they were defined as the responses consistent with function transfer. Corrections were excluded from the computation of percentage correct on training trials, and percentages were computed for blocks of two cycles.

The pretrained performance of each subject generally remained at high levels on training trials (gray vertical bars in Figure 3). Three subjects showed transfer scores of

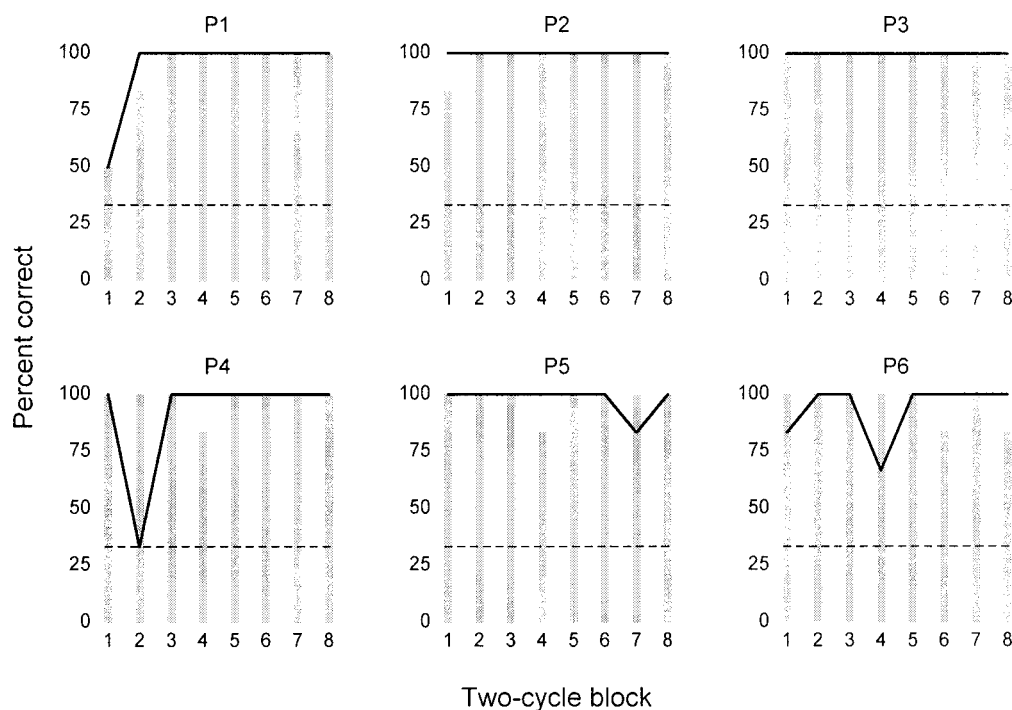


Fig. 3. Percentages of correct responses on training trials (gray vertical bars) and test trials (solid lines) for each subject in Experiment 1. See text for details. Dashed horizontal lines indicate the percentage of correct responses expected on test trials (33%) if responding were random from trial to trial.

100% on the first block of training and testing, and all subjects did so by the end of the session (solid lines in Figure 3). Stimulus contiguity produced function transfer quickly and reliably.

EXPERIMENT 2

The findings of Experiment 1 notwithstanding, the notion that function transfer in stimulus-equivalence studies (e.g., Catania et al., 1989) derives from stimulus pairings could be questioned on logical grounds. The matching-to-sample trials of stimulus-equivalence experiments, for example, typically include all comparison stimuli; hence on any such trial, the sample is temporally contiguous not only with the correct comparison but also with the incorrect ones. In such conditions, why should function transfer be restricted to the stimuli actually matched with each other? Two factors may explain the specificity of function transfer in matching-to-sample procedures. First, differential observing of the comparison stimulus selected by the

subject could increase its salience with respect to that of the other comparisons (Rehfeldt & Hayes, 1998). Second, some Pavlovian conditioning data (Holland, 1981; Ward-Robinson & Hall, 1996) suggest that associating two stimuli B and C with a common context A can increase the potency of a B-C correlation that would be ineffective otherwise (Tonneau & Sokolowski, 1997). Matching one comparison with the sample provides such a shared context. Similar events (pointing, moving the cursor onto the stimulus, etc.) occur with respect to the sample and the chosen comparison, but not with respect to the other stimuli present on the same trial. This common context, applied to the sample and the correct comparison, might increase their joint salience beyond that of the other stimulus pairs.

Even if such contextual factors operate, Sidman (1994) has remarked that in operant procedures, stimulus duration, being determined by the subject's own behavior, varies from trial to trial and "is seldom fixed at a value that would encourage respondent con-

ditioning" (p. 398). Yet the actual time ranges compatible with Pavlovian conditioning depend on numerous response and stimulus parameters (see Gallistel, 1990). How the temporal parameters of stimulus-pairing procedures affect the transfer of discriminative functions in humans is unclear.

These issues were addressed in Experiment 2 through the use of yoking techniques. Two groups of subjects participated. The subjects in the first group were exposed to a standard matching-to-sample arrangement. The subjects in the second group were exposed to stimulus pairings in which the sample and one of the three comparison stimuli (present concurrently on the screen) were tagged with a contextual cue yoked to the responses and latencies of subjects in the first group. If pairing explanations of function transfer are correct, then transfer of discriminative function should be observed even in such conditions—with concurrent comparison stimuli and variable stimulus duration.

METHOD

Subjects

Twelve students participated as subjects. A first group of 6 subjects (M1 to M6) was exposed to the *matching condition*; a second group of 6 subjects (Y1 to Y6) was exposed to the *yoked condition*. Each group was comprised of 3 boys and 3 girls, with ages ranging from 12 to 13 years.

Procedure

Matching condition. The procedure was identical to that of Experiment 1, with the exception that on each cycle of the training-and-testing phase, the pairing trials (Figure 2) were replaced by matching-to-sample trials involving B1, B2, and B3 and C1, C2, and C3. The instructions that started the training-and-testing phase were adjusted accordingly:

Now, apart from pressing keys, you'll need to use the computer mouse to choose among various figures. To select a figure (for example, the black square that you can see at the bottom of this screen), you'll need to use the mouse to move the cursor onto this figure and then press any mouse button. You may try now with the black square. Click on the black square.

A black square (2 cm by 2 cm) was visible at

the bottom of the screen. When the subject clicked on the square, the previous instructions were replaced with the following ones:

See? Choosing a figure with the mouse is as simple as this. After choosing a figure, the computer will tell you whether you were right or not. Your task is simple, but every error counts. It is better to respond SLOWLY and carefully than to respond quickly and make a mistake. Even if you make mistakes at first, you'll learn the correct choices in the course of the experiment. Click with the mouse on the rectangle labeled "Ready" to see the remaining instructions.

When the subject clicked on the rectangle labeled "Ready," the warning message about nonspecific feedback (see Experiment 1) appeared. Once this message was read, the training-and-testing phase began.

Each training-and-testing cycle comprised three training trials (pending correction), six matching trials (pending correction), and three test trials. Training and test trials were as in Experiment 1. On each matching trial, a colored shape (B1, B2, or B3) appeared at the center of the screen, with the prompt "Click on this figure" on its left. When the subject clicked on the colored shape, the prompt vanished and three comparison stimuli (C1, C2, and C3) appeared in a row 1 cm above the sample, 11 cm separating the leftmost and rightmost comparisons. Also, a prompt at the top of the screen instructed the subject to "Choose one of these three figures with the mouse." Once the subject clicked on a comparison stimulus, the prompt was replaced by a message commenting on the subject's choice (either "Good!" or "Wrong. Try again," depending on whether the chosen comparison was correct or not). After 2 s, the screen went blank for a 1-s intertrial interval.

On each cycle, the order of the matching-to-sample trials that involved B1, B2, or B3 was random within blocks of three; the positions of the comparisons on the screen were also randomized. If the subject's matching response was incorrect, the same matching-to-sample configuration was repeated until the subject made two correct responses in a row. There are six ways to match three sample stimuli (B1, B2, and B3) to three comparisons (C1, C2, and C3); each possible way was assigned to a different subject.

Yoked condition. The procedure was identical to that of Experiment 1, with the exception that on each cycle of the training-and-testing phase the pairing trials (Figure 2) were replaced by a varying number of *yoked pairing trials* involving B1, B2, and B3 and C1, C2, and C3. These trials were modeled after the matching-to-sample trials (including correction trials) of a subject in the matching condition (hereafter, the “model”).

Each yoked pairing trial started with a colored shape (B1, B2, or B3) identical to that shown to the model on the corresponding matching trial. This shape appeared at the center of the screen, with a contextual cue (a 3.5 cm by 3.5 cm green square frame centered around the colored shape) and the word “look” 3 cm below it. The colored shape, the word “look,” and the contextual cue remained visible on the screen for a time identical to that taken by the model to click on this shape on the matching trial. Then the three geometric figures (C1, C2, and C3) were added on the screen in the positions that they occupied in the matching trial of the model. After a delay identical to the one taken by the model to choose a comparison stimulus on that particular trial, the contextual cue was added to the figure chosen by the model; if, for example, the model had chosen C2 on the matching trial, then the contextual cue was added to C2. After 2 s, the screen went blank for a 1-s intertrial interval. The yoked pairing trials of subjects Y1 to Y6 were yoked to the matching trials of models M1 to M6, respectively.

RESULTS AND DISCUSSION

The number of pretraining trials (including corrections) necessary to reach three correct key presses in a row averaged 17.5 (range, 3 to 62). The upper panel of Figure 4 presents the percentages of correct responses (computed as in Experiment 1) on the training and test trials of each subject in the matching condition, along with their percentages of correct choices on matching trials (computed by blocks of two cycles and excluding correction trials). The pretrained performance of each subject generally remained at high levels on training trials (gray vertical bars), and percentage correct on matching trials quickly reached 100% (dotted lines). Most subjects showed high transfer

scores and all obtained 100% correct responses by the end of the session (solid lines).

The lower panel of Figure 4 shows the percentages of correct responses on training and test trials for each subject in the yoked condition. Pretrained performance generally remained at high levels on training trials (gray vertical bars). All subjects started the training-and-testing session with less than perfect transfer scores but achieved 100% correct responses by the end of the session (solid lines). These results suggest that function transfer can be produced by stimulus pairings even though all comparisons are present on each trial and stimulus duration varies from trial to trial.

EXPERIMENT 3

The results of Experiment 2 further support the hypothesis that function transfer among stimuli matched to one another (e.g., Catania et al., 1989; Lazar, 1977) takes place because these stimuli are paired and not because of operant responses and their reinforcement. However, many studies of stimulus equivalence have documented function transfer among stimuli that never appeared together. In a study by Hayes et al. (1991, Experiment 2), for example, stimulus A was matched with stimulus B on some trials ($A \rightarrow B$) and with stimulus C on different trials ($A \rightarrow C$). Hence B and C were never simultaneously present. Yet function transfer from C to B was observed.

On the basis on such data, Hayes et al. (1991) have argued that in at least some operant studies of function transfer, “direct pairing is unlikely as an explanation for the results” (p. 126), and thus complex operant processes may be implicated (Hayes, 1994). Clearly, function transfer from B to C after $A \rightarrow B$, $A \rightarrow C$ matching cannot be due to direct stimulus pairings. However, direct stimulus pairings are not necessary to Pavlovian conditioning either, as studies of second-order and sensory preconditioning (e.g., Rizley & Rescorla, 1972) demonstrate. Furthermore, function transfer from C to B after $A-B$, $A-C$ pairing trials has been documented in the Pavlovian literature. In an experiment by Holland (1981), for example, rats were first exposed to stimulus pairings ($A-B$) be-

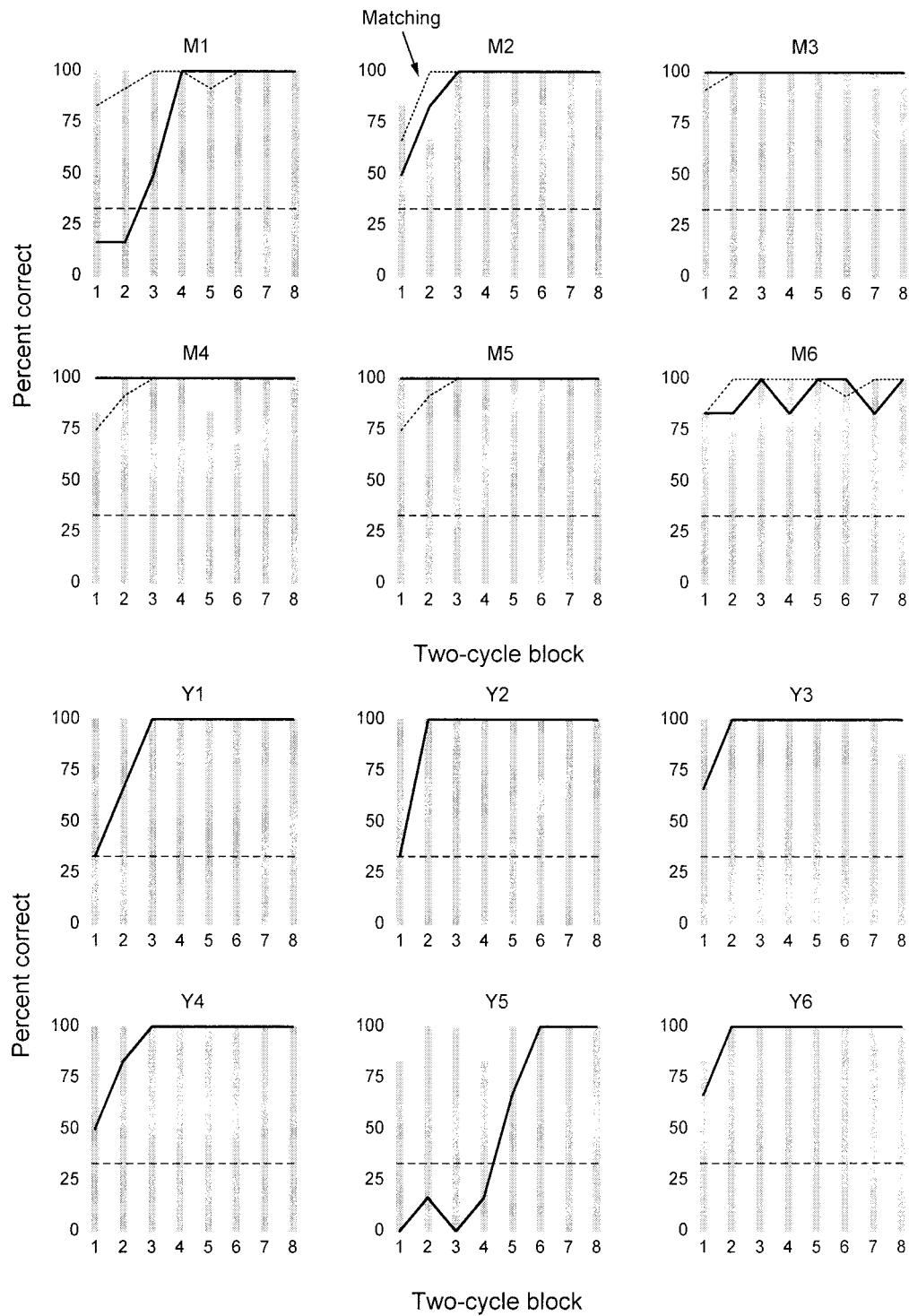


Fig. 4. Top: Percentages of correct responses on training trials (gray vertical bars), matching trials (dotted lines), and test trials (solid lines) for each subject in the matching condition of Experiment 2. Bottom: Percentages of correct responses on training trials (gray vertical bars) and test trials (solid lines) for each subject in the yoked condition of Experiment 2. Dashed horizontal lines indicate the percentage of correct responses expected on test trials (33%) if responding were random from trial to trial.

tween a neutral stimulus (A) and food (B). Stimulus A was later paired with a toxin (A-C). In the final stage of the experiment, the rats displayed a conditioned aversion to food B, as if it had itself been paired with toxin C. Similar A-B, A-C effects have been obtained by Ward-Robinson and Hall (1996).

Experiment 3 was designed to document the transfer of discriminative function from C to B through A-B, A-C indirect pairings. Observing function transfer through A-B, A-C pairings would further support the notion that function transfer in operant, stimulus-equivalence experiments can arise from stimulus pairings.

METHOD

Subjects

Six students (3 boys and 3 girls, with ages ranging from 12 to 15 years) participated as subjects. Their names were coded from IN1 to IN6.

Procedure

The procedure was identical to that of Experiment 1, with the exception that on each cycle of the training-and-testing phase the pairing trials involved *indirect pairings* (A-B, A-C). These trials involved B1, B2, and B3 and C1, C2, and C3, as well as the contextual cues labeled A1, A2, and A3 in Figure 1 (a green hexagon, a gray star, and a red rectangle).

Each training-and-testing cycle included three training trials (pending correction), 12 trials of indirect pairing, and three test trials. The training and testing trials were as in Experiment 1. On each indirect pairing trial, a contextual cue (A1, A2, or A3) appeared at the center of the screen with the word "look" 3 cm below it and another stimulus (B1, B2, B3, C1, C2, or C3) 1.5 cm above it. After 1.5 s, the screen went blank for a 2.5-s intertrial interval. Each of the three contextual cues (A1, A2, or A3) appeared four times over the 12 trials of indirect pairings, twice with one of the B stimuli (B1, B2, or B3) and twice with one of the C stimuli (C1, C2, or C3). The order of the indirect pairing trials on each cycle was random.

The results of pilot studies and Experiments 1 and 2 showed no tendency of B1, B2, and B3 to evoke the responses A, G, and L in

any particular order. In Experiment 3, therefore, B1 was indirectly paired to C1, B2 to C2, and B3 to C3 for all subjects. However, each of the six possible ways to associate the contextual cues (A1, A2, and A3) with the B1-C1, B2-C2, and B3-C3 pairs was assigned to a different subject.

RESULTS AND DISCUSSION

The number of pretraining trials (including corrections) necessary to reach three correct key presses in a row averaged 14.7 (range, 11 to 20). Figure 5 presents the percentages of correct responses on training and test trials for each subject, the data being computed as in the previous experiments.

With the exception of IN6, whose pretrained performance deteriorated somewhat at the start of this phase, pretrained performance remained close to 100% on training trials (gray vertical bars). All subjects except IN1 achieved 100% correct responses on their last block of test trials (solid lines). The number of subjects reaching a transfer score of 100% by the end of the session (5 out of 6) was significantly higher than expected by chance (one-tailed binomial test, $p = .167$, $n = 6$; P -value $\approx .0007$).

Why 1 subject (IN1) failed to show function transfer could not be determined. This outcome is not specific to our pairing procedures, however, because similar failures have been reported in the operant literature on function transfer (e.g., Hayes et al., 1991, Experiment 1, Subject 9; Lazar, 1977, Subject S-3).

EXPERIMENT 4

Our findings suggest that some common objections to Pavlovian accounts of function transfer should be reevaluated. Discriminative transfer can take place with all comparisons present concurrently (Experiment 2) and through indirect stimulus pairings (Experiment 3). However, the relevance of these results to a Pavlovian account could be questioned if they involved an alternative mechanism such as verbal mediation (Lowe & Horne, 1996, pp. 319-320). Consider Experiment 1, for example. When shown a stimulus pair such as B-C, the subjects could name each stimulus in succession ("B," "C") and later elaborate more complex verbal chains

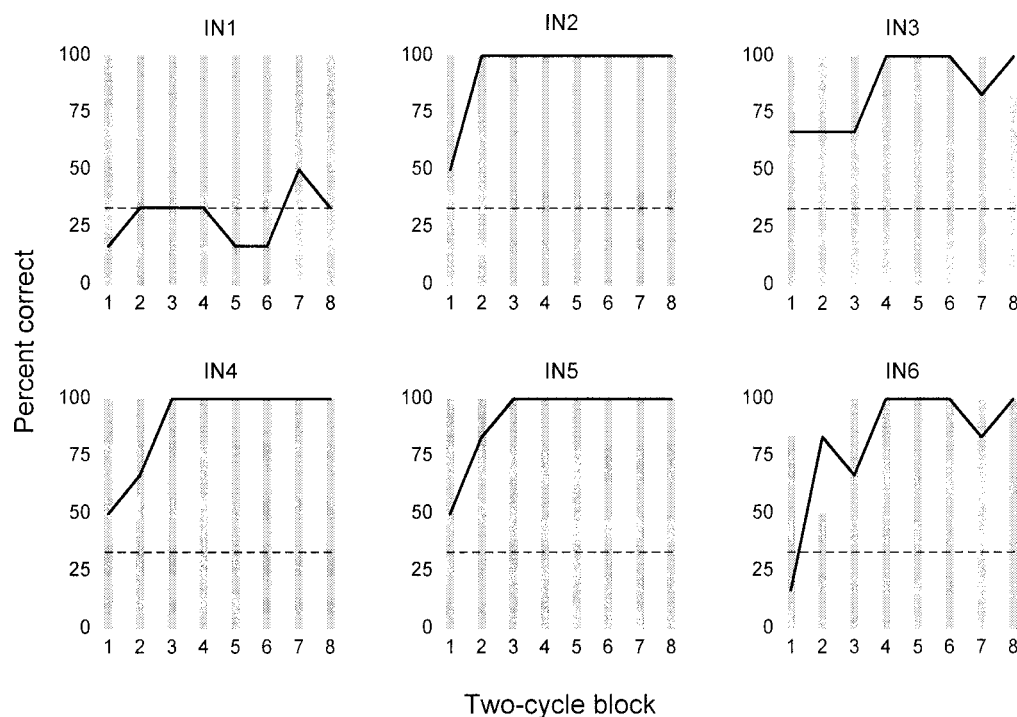


Fig. 5. Percentages of correct responses on training trials (gray vertical bars) and test trials (solid lines) for each subject in Experiment 3. Dashed horizontal lines indicate the percentage of correct responses expected on test trials (33%) if responding were random from trial to trial.

including both “B” and “C.” These verbal sequences could then mediate behavior on test trials with the B stimuli (see Horne & Lowe, 1996). In this type of account, no Pavlovian process is needed because the transfer results arise from the chaining properties of verbal behavior itself.

Although the hypothesized verbal mediators may be gestural in the case of deaf, anarthric, or developmentally delayed individuals (Lowe & Horne, 1996), in fluent speakers the relevant verbal activities presumably involve the vocal apparatus (see Horne & Lowe, 1996). One way to test explanations that appeal to covert vocal activities is to require subjects to repeatedly speak aloud an irrelevant verbal item during the task of interest (e.g., Gupta & MacWhinney, 1995). That such a procedure actually interferes with covert verbal behavior is suggested by the fact that such interventions eliminate the effects of word length in tasks of working memory (e.g., Baddeley, Lewis, & Vallar, 1984; Baddeley, Thomson, & Buchanan, 1975), effects that are of-

ten attributed to verbal mediation (e.g., Hulme, Silvester, Smith, & Muir, 1986).

Following this logic, in Experiment 4 we asked subjects to repeat “blah” continuously during the stimulus-pairing trials. If function transfer takes place under such conditions, then at least part of the effects of stimulus pairings in our experiments must arise from processes other than vocal mediation.

METHOD

Subjects

Six students (3 boys and 3 girls, with ages ranging from 11 to 15 years) participated as subjects. Their names were coded from V1 to V6.

Procedure

The procedure was identical to that of Experiment 1, with the following exceptions. First, before starting the session the subject was taught to repeat “blah” without pausing for at least 6 s. The subject was asked to imitate the experimenter inhaling deeply and

then repeating “blah” continuously at a rate of at least four syllables per second. The experimenter encouraged the subject and modeled performance until vocalization rate proved adequate.

Second, the experimenter remained in the room during the session, employing the computer’s mouse to move from one screen of instructions to the next, to start the trials of stimulus pairing, and to resume training and testing after a period of rest. The instructions of Experiment 1 were adjusted accordingly; all sentences that mentioned using the mouse were deleted. Aside from the warning about nonspecific feedback (see Experiment 1), the training-and-testing phase began with the following instructions:

Now, apart from pressing keys, you’ll need to LOOK at some figures that the computer will show you on the screen. You’ll need to pay attention. While looking at these figures you’ll need to repeat BLAH BLAH BLAH continuously. The experimenter will tell you when to start and when to stop.

After reading these instructions to the subject, the experimenter again modeled continuous vocalizations for at least 6 s. Then the training-and-testing phase started.

Each training-and-testing cycle included three training trials (pending correction), two pairing trials, and three test trials. The training and test trials were as in Experiment 1. The start of a pairing trial was signaled on the screen by the word, “stop,” at which point the subject was asked to imitate the experimenter inhaling and then repeating “blah” continuously. Once the subject started to say “blah,” the experimenter clicked on the mouse to present stimulus pairs. Each pairing trial comprised three pairs (e.g., A1–B1, A2–B2, and A3–B3), each pair involving the 1-s presentation of an A stimulus at the center of the screen with a B stimulus 0.5 cm above it, followed by a 0.75-s interpair interval. The subject and the experimenter stopped vocalizing after at least 1 s had elapsed since the last stimulus pair; a few seconds were left after each pairing trial to allow the subject to breathe easy and relax. The first pair of each trial involved B1, the second pair B2, and the last pair B3; each of the six possible ways of pairing the B and C stimuli was assigned to a different subject.

On training and test trials, the experimenter stood motionless about 1 m behind the subject’s back to minimize the possibility of unintentional cueing. Finally, to avoid subject fatigue, the training-and-testing phase comprised only eight cycles interrupted by a rest period after the fourth cycle. Subject vocalizations were tape recorded.

RESULTS AND DISCUSSION

The number of pretraining trials (including corrections) necessary to reach three correct key presses in a row averaged 11.3 (range, 3 to 16). Figure 6 presents the percentages of correct responses on training and test trials for each subject, the data being computed as in the previous experiments.

Pretrained performance usually remained close to 100% on training trials (gray vertical bars). All subjects except V5 obtained 100% correct responses on their last block of test trials (solid lines); V5’s data, although suggestive of function transfer, were inconsistent. The number of subjects reaching a transfer score of 100% (5 out of 6) was significantly higher than expected by chance (one-tailed binomial test, $p = .167$, $n = 6$; P -value $\approx .0007$). Vocalization rates (rounded to the nearest half-syllable) during pairing ranged from 4.5 to 6.5 syllables per second. The results show that stimulus pairings can induce function transfer even when covert verbal processes presumably are restricted by articulatory suppression.

GENERAL DISCUSSION

Summary of the results. Experiment 1 documented the transfer of discriminative functions through direct stimulus pairings (B–C). Importantly, pairing proceeded in the absence of programmed consequences (although, of course, differential consequences were provided on training trials). Experiment 2 showed that function transfer persisted even though all comparison stimuli were present on each trial and even though stimulus duration, as a result of yoking, varied from trial to trial. Presumably, Sidman’s (1994) objections to a Pavlovian account of function transfer, although relevant to precisely timed respondent behaviors (such as the rabbit’s eye blink), do not apply to the transfer of discriminative functions through stimulus pair-

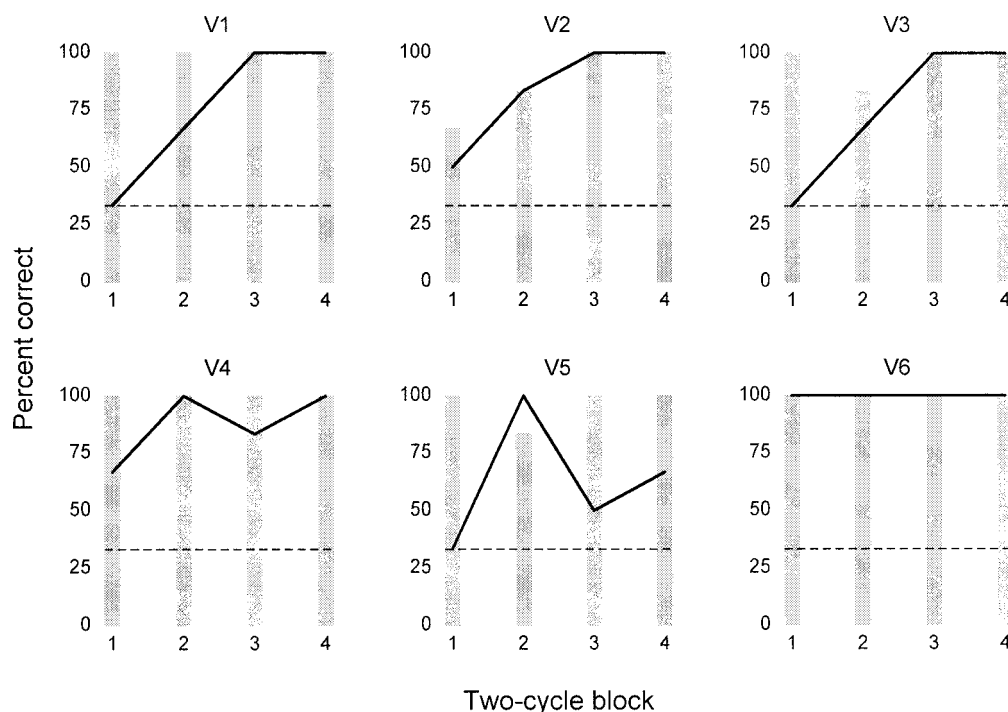


Fig. 6. Percentages of correct responses on training trials (gray vertical bars) and test trials (solid lines) for each subject in Experiment 4. Dashed horizontal lines indicate the percentage of correct responses expected on test trials (33%) if responding were random from trial to trial.

ings (see Staats, 1966, 1968). Experiment 3 demonstrated function transfer through indirect stimulus pairings, using an A-B, A-C pairing structure analogous to Pavlovian procedures (e.g., Ward-Robinson & Hall, 1996). Our aim in this experiment was to approximate the temporal and stimulus parameters of the function-transfer studies that rely on matching to sample (e.g., Hayes et al., 1991). Experiment 4 demonstrated that continuous vocalizations during direct pairing (B-C) did not preclude function transfer.

A Pavlovian account of function transfer. The effects reported here are analogous to Pavlovian findings of function transfer through direct and indirect stimulus pairings (e.g., Hall, 1996). Our data are consistent with the hypothesis that function transfer is a Pavlovian process (Boelens, 1990; Rehfeldt & Hayes, 1998; Staats, 1966, 1968; Tonneau, 1993) in which environmental pairings transfer a wide range of stimulus functions, respondent or otherwise (Turkkan, 1993). From this perspective, the main role of operant reinforcement in our experiments was to determine

which responses predominated in the presence of particular stimuli, and therefore which responses transferred to other stimuli through environmental pairings (Tonneau, 2002).

Operant variables such as discriminative stimuli may also contribute to Pavlovian transfer by fostering observing responses and attending to the relevant stimulus pairs (see Rehfeldt & Hayes, 1998). In a study by Dougher et al. (1994), for example, an operant task was used during Pavlovian conditioning to keep the subjects looking at the stimuli. In our study, we employed instructions and/or the word "look" to guarantee that the subjects would attend to stimulus pairings. The role of such manipulations in promoting function transfer in humans deserves more investigation. Merely demonstrating an effect of these variables would provide little support for an operant account of function transfer over a Pavlovian one, however, because attentional variables are also known to influence Pavlovian conditioning

(e.g., Kaye & Pearce, 1984; Swan & Pearce, 1988).

One theoretical advantage of a Pavlovian perspective on function transfer is that it connects recent studies of function transfer in humans (e.g., Hayes et al., 1991; Markham & Markham, 2002) to an extensive literature (e.g., Pavlov, 1927; Turkkan, 1989, 1993) on which current operant theories are largely silent. Below we examine the implications of our data for operant theories of function transfer.

The equivalence relation framework. Sidman (1994) proposed that function transfer in Pavlovian settings was merely a type of “stimulus equivalence” emerging from stimulus pairings. However, this particular account of function transfer misapplied set theory (see Tonneau, 2001) and therefore cannot explain our results. In updating his earlier account, Sidman (2000) dropped all reference to Pavlovian factors, maintaining instead that function transfer is merely an aspect of “equivalence relations,” that in turn arise from reinforcement contingencies. According to this new account, operant reinforcers must be present and delivered after particular responses are emitted in conjunction with stimulus pairs. Operant reinforcement was not programmed into the stimulus-pairing procedures of the present experiments, however, and it is not clear what form of unprogrammed reinforcement might be assumed. The notion that stimuli reinforce looking at them (cf. Schroeder & Holland, 1969), for example, cannot easily explain the data of Experiment 2, in which all comparison stimuli were simultaneously present and presumably looked at.

Operant frames and Pavlovian processes. Operant approaches such as relational frame theory (Hayes, Barnes-Holmes, & Roche, 2001; cf. Boelens, 1994) hold that function transfer can be reinforced (or punished) by its consequences and generalize across settings. This hypothesis suggests, for example, that no function transfer would have been observed in the present study if our subjects had not previously exhibited function transfer in situations that involved stimulus pairs, and if function transfer across stimulus pairs had not been reinforced (Boelens, 1994). This possibility cannot be ruled out by the present results and deserves more investigation.

Yet a theoretical question remains. Can such operant accounts explain our data without appealing to a Pavlovian process? By definition, these accounts assume that function transfer occurs at least one time in the subject’s preexperimental history (due to some unspecified behavioral process X) to be later reinforced or punished. Reinforcing a phenomenon X, however, only means to increase its rate through its consequences, not to transform its nature; thus the process X that allowed function transfer to take place *before* function transfer was first reinforced must still be the process X operative thereafter (for more discussion of this logical issue see Tonneau, 2001, 2002). What is this unspecified process X, and what are its properties? Pavlovian processes are a likely candidate (e.g., Barnes, 1994; Tonneau, 2002).

Classes versus stimuli. Other operant accounts of function transfer appeal to the metaphor of class formation through shared functions (see Dougher & Markham, 1996). Applied to our data, this notion implies that contextual cues (such as the word “look,” or perhaps the pairing procedure itself) could induce a “partition” of the stimuli into different classes (presumably B1–C1, B2–C2, and B3–C3) and promote function transfer within these classes. Note, however, that this account requires nonoverlapping classes; hence, each of the assumed classes must be correlated with a different cue (as in Experiment 3, in which the contextual cues A1, A2, and A3 were associated with the pairs (B1, C1), (B2, C2), and (B3, C3), for example).

In Experiment 1, however, the same contextual cues (e.g., the word “look,” or the pairing procedure itself) were equally associated with all stimuli. The word “look” was presented with B1 and C1 (on B1–C1 trials), but *also* with B2, C2, B3, and C3 (on B2–C2 and B3–C3 trials); that some of these stimuli were sometimes present simultaneously, and others not, is irrelevant to the metaphor of class formation through shared contextual cues (e.g., Markham & Markham, 2002). Far from predicting our data, therefore, the proposed account defines a single class (B1, C1, B2, C2, B3, C3) and is incompatible with stimulus-specific transfer (say, from C1 to B1 only). In contrast, a Pavlovian account applies to our data because it appeals to pairings

among stimuli and not classes; that is, the account is not class-based.

Verbal behavior and covert rules. One last class of operant-based accounts of our data relies on (presumably covert) verbal behaviors and rule formulations (Horne & Lowe, 1996). As discussed above, a verbal account of function transfer in humans can explain the data of Experiments 1 to 3 without invoking Pavlovian factors. In Experiment 4, however, the subjects were vocalizing at a rate of at least four syllables per second during stimulus pairings. The subjects could emit verbal behaviors *after*, but presumably not *during*, each pairing trial. In this case, a verbal account of function transfer must be supplemented (at least) by a process X that can still function while vocalization is impaired, that allows the subject to "keep track" of the pairs shown on the pairing trial, and that allows later verbal behavior to reflect adequately what was paired with what. That X is sensitive to stimulus contiguity is consistent with a Pavlovian process (Turkkan, 1989).

Importantly, a strictly Pavlovian account is compatible with an adverse effect of vocalization on function transfer (as in subject V5, perhaps), because interference due to concurrent stimulation has been amply documented in Pavlovian settings (e.g., James & Wagner, 1980). What is incompatible with a strictly verbal account of function transfer is the persistence of the latter under articulatory suppression (as shown in Figure 6).

A difficulty with this conclusion is that the relevant covert activities may not have been completely suppressed by concurrent vocalization (see Peterson, 1969). Alternatively, one might argue that the processes operative in Experiment 4 are at least in part Pavlovian but differ from those operative in the other experiments. Pursuing the first solution will require specifying the nature of the covert activities that could have survived concurrent vocalization. The second solution is uneconomical, and both solutions seem ad hoc.

Conclusion. The most parsimonious and coherent account of our results is Pavlovian (e.g., Tonneau, 2001). We hope that the present data will encourage researchers to clarify the role of Pavlovian factors in producing phenomena that have been attributed to operant reinforcement in the absence of adequate control conditions. That function trans-

fer in humans, as opposed to other animals, arises from unspecified operant variables remains a logical possibility, but arguing so remains of limited theoretical value until a coherent and refutable alternative to a Pavlovian account is developed. Although the role of Pavlovian processes in complex human behavior remains to be determined, the issue is certainly worth examining in detail.

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